

EXHIBIT 5

Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides

Kara E. Rudolph, PhD, MPH, MHS, Elizabeth A. Stuart, PhD, Jon S. Vernick, JD, and Daniel W. Webster, ScD, MPH

Homicide was the second leading cause of death for individuals aged 15 to 34 years in the United States from 1999 to 2011¹ and the second leading contributor to racial disparities in premature mortality among men.² Firearms are used in more than two thirds of homicides in the United States,³ and firearm availability, especially to high-risk groups (e.g., perpetrators of domestic violence and violent misdemeanors),^{4,5} is positively associated with homicide risks.^{6,7}

Given the importance of firearms in lethal violence, many federal and state policies have been designed to prevent individuals with a history of violence, criminal behavior, substance abuse, or serious mental illness from accessing firearms. Federal law mandates that individuals who purchase firearms from federally licensed dealers pass a background check, but sales by private, unlicensed sellers are exempt. Eighteen states and the District of Columbia require handgun purchasers from private, unlicensed sellers to pass background checks. Ten of these states and the District of Columbia strengthen the background check requirement with a permit-to-purchase (PTP) law, although 4 do not require a new background check at the time of purchase.⁸ PTP laws require individuals to obtain a permit or license to purchase a handgun (from both licensed retail dealers and private sellers) that is contingent upon passing a background check and, in some cases, completing safety training. In 8 states, individuals must apply for a PTP in person at the law enforcement agency that initiates the background checks and issues permits. In the other 42 states, pre-gun-sale background checks are initiated through a licensed gun dealer, although there are significant differences among these policies. Table A (available as a supplement to this article at <http://www.ajph.org>) summarizes the status of these laws by state.

We conducted this study to estimate the impact of Connecticut's 1995 PTP law. This

Objectives. We sought to estimate the effect of Connecticut's implementation of a handgun permit-to-purchase law in October 1995 on subsequent homicides.

Methods. Using the synthetic control method, we compared Connecticut's homicide rates after the law's implementation to rates we would have expected had the law not been implemented. To estimate the counterfactual, we used longitudinal data from a weighted combination of comparison states identified based on the ability of their prelaw homicide trends and covariates to predict prelaw homicide trends in Connecticut.

Results. We estimated that the law was associated with a 40% reduction in Connecticut's firearm homicide rates during the first 10 years that the law was in place. By contrast, there was no evidence for a reduction in nonfirearm homicides.

Conclusions. Consistent with prior research, this study demonstrated that Connecticut's handgun permit-to-purchase law was associated with a subsequent reduction in homicide rates. As would be expected if the law drove the reduction, the policy's effects were only evident for homicides committed with firearms. (*Am J Public Health*. 2015;105:e49–e54. doi:10.2105/AJPH.2015.302703)

law strengthened background check requirements, especially for handguns purchased by private sellers. In addition, it raised the handgun purchasing age from 18 to 21 years and required any prospective handgun purchaser to apply for a permit in person with the local police and complete at least 8 hours of approved handgun safety training.

METHODS

To estimate the effect of Connecticut's PTP law on homicides, we compared Connecticut's homicide rates observed after the law's implementation to the rates we would have expected had the law not been implemented (the counterfactual). To estimate the counterfactual, we used longitudinal data from a weighted combination of comparison states with no PTP law change (henceforth, Connecticut's synthetic control) identified based on the ability of their prelaw homicide trends and covariates to predict prelaw homicide trends in Connecticut.

States that were considered as potential comparison states for Connecticut were those that did not have a PTP law in 1995 and

therefore were "at risk" for implementing a new PTP law in 1995. Ten states (Hawaii, Illinois, Iowa, Missouri, Massachusetts, Michigan, Nebraska, New Jersey, New York, and North Carolina) and the District of Columbia were excluded from the pool of possible controls because they implemented a PTP law prior to 1995. We used outcome and annual covariate data from Connecticut and each of the 39 states in the control pool from 1984 to 2005. We concluded the postlaw period in 2005 to limit counterfactual predictions to 10 years, as has been done previously.⁹

Outcomes

We examined 2 outcomes—firearm-specific homicide rates and non–firearm-specific homicide rates (number of homicides per 100 000 state residents)—obtained from compressed mortality data from the Centers for Disease Control and Prevention's Wide-ranging Online Data for Epidemiologic Research database (<http://wonder.cdc.gov/mortSQL.html>). We expected the impact of the PTP law—if any—to be limited to homicides committed with firearms.

RESEARCH AND PRACTICE

Covariates

Annually measured state-level covariates and their sources follow. Population size, population density (log-transformed), proportion aged 0 to 18 years, proportion aged 15 to 24 years, proportion Black (log-transformed), proportion Hispanic (log-transformed), proportion aged 16 years or older living at or below poverty, and income inequality as measured by the Gini coefficient were from the US Census Bureau. Average per capita individual income and number of jobs per adult were from the Bureau of Economic Analysis. Proportion living in metropolitan statistical areas, law enforcement officers per 100 000 residents, and robberies per 100 000 residents were from the Federal Bureau of Investigation's Crime in the United States publications. The Census of Governments provided data on annual expenditures for law enforcement (current operation and capital outlay).

Statistical Analysis

We used the synthetic control group approach⁹ to create a weighted combination of states that exhibited homicide trends most similar to Connecticut's prior to the law's implementation (1984–1994). This weighted combination of states can be thought of as a "synthetic" Connecticut, whose homicide trends during the postlaw period predict the post-1995 trends that Connecticut would have experienced in the absence of the law change.

The algorithm for creating the weights has been described previously.⁹ The vector of weights minimized the mean squared prediction error (MSPE) between the homicide rates of Connecticut during the prelaw period and the weighted vector of outcomes and covariates of the control pool states during the prelaw period.⁹ No data from 1995 or thereafter were used in creating the weights and synthetic control.

After creating the weights using the Synth package in R,¹⁰ we compared homicide rates between Connecticut and its synthetic control in the 10 years after the PIP law was implemented (from 1996 to 2005). We excluded 1995 because the law was not implemented until October of that year. We excluded 2001 from the nonfirearm homicide analysis because of the large increase in deaths attributable to the 2001 terrorist attacks, which had

a disproportionate impact on Connecticut residents. The estimated number of homicides prevented by the law from 1996 to 2005 was calculated by multiplying the difference in homicide rates between Connecticut and its synthetic control by Connecticut's population size (in 100 000s) each year and summing across the years.

Statistical significance was assessed using a permutation-based test—also called a placebo or falsification test—that is similar to the Fisher exact test.^{9,11} For each outcome, we repeated the analysis where we considered each of the 39 states in the control pool as the "treated" state and created a synthetic control for each of these states. We calculated the proportion of control states with an estimated rate of prevented homicides that was as extreme as or more extreme than the estimated rate prevented for Connecticut. This proportion was akin to the *P* value and indicated how unusual Connecticut's estimated effect was compared with the states in the control pool.

However, not every control state's homicide trend can be well approximated by a synthetic control. Lack of fit was determined by greater MSPE, which is the average of the squared differences between homicide rates in the "treated" state and its synthetic control during the prelaw period. In cases of large MSPE, it is not appropriate to use the synthetic control as a comparison. Consequently, we calculated the proportions of control states with results as extreme or more extreme than Connecticut for 3 separate control pools, including control

states whose MSPE from their synthetic control was no more than (1) 20×, (2) 5×, and (3) 2× that of Connecticut's synthetic control MSPE. This entire analysis process was conducted twice: once for firearm homicides and once for nonfirearm homicides. We used R version 3.0.2 for all analyses.¹²

Sensitivity Analysis

In the data available as a supplement to the online version of this article, we considered an alternative approach in which we compared Connecticut's homicide rate trends to the 39 control states' average trends that were mean-shifted to the scale of Connecticut's homicide rates.

RESULTS

Using the predictive covariates as well as prelaw outcome data, we constructed a synthetic control for Connecticut for each of the 2 outcomes of interest. States with a nonzero weight contributed to the synthetic control and are listed in Table 1. Table 1 also shows how well the synthetic control approximated Connecticut's homicide rates during the prelaw period, as measured by MSPE. The last row of this table shows that the synthetic control was a better fit than a simple average of all the states in the control pool. For example, in the case of firearm homicides, the synthetic control had an MSPE of 0.157, which is an order of magnitude less than the MSPE if a simple average of all control states had been used.

TABLE 1—States With Nonzero Weights In the Synthetic Connecticut for Firearm and Nonfirearm Homicide Rates: 1996–2005

| State | Weight | |
|---|-------------------|----------------------|
| | Firearm Homicides | Nonfirearm Homicides |
| California | 0.036 | 0.000 |
| Maryland | 0.147 | 0.110 |
| Nevada | 0.087 | 0.121 |
| New Hampshire | 0.005 | 0.724 |
| Rhode Island | 0.724 | 0.046 |
| MSPE synthetic control/all control states | 0.157/1.633 | 0.090/0.740 |

Note. MSPE = mean squared prediction error. Thirty-nine states were included in the pool of possible controls. Ten states with a similar law implemented prior to 1995 were not included: Hawaii, Illinois, Iowa, Missouri, Massachusetts, Michigan, Nebraska, New Jersey, New York, and North Carolina.

Table B (available as a supplement to this article at <http://www.ajph.org>) shows descriptive statistics for each of the covariates found to be predictive of homicide rates during the prelaw period. These variable summaries are provided for Connecticut, the pool of control states, and Connecticut's synthetic control optimized for (1) firearm and (2) nonfirearm homicides.

Figures 1 and 2 compare firearm and nonfirearm homicide rates over time between Connecticut and its synthetic control. The average homicide rates over the study period for all states in the control pool are included for reference. Figure 1 shows that firearm homicide rates for Connecticut and its synthetic control tracked together prior to the law's implementation in October 1995; this is also evidenced by the low MSPE shown in Table 1. However, beginning in 1999, the rates diverged markedly. Connecticut's firearm homicide rate continued to decline before leveling off in the early 2000s, whereas its synthetic

control's firearm homicide rate leveled off approximately 5 years earlier. Summing the differences between Connecticut and its synthetic control from 1996 to 2005, we estimated the law to be associated with 296 fewer firearm homicides during this period, a reduction of 40% relative to the counterfactual.

The permutation tests were consistent with this graphical intuition and indicated that Connecticut's divergent firearm homicide trend during the postlaw period was statistically significant. None of the 30 potential control states with an MSPE no more than 5× that of Connecticut's had firearm homicide trends that diverged as widely from their synthetic controls as Connecticut's did (Table 2).

Figure 2 shows nonfirearm homicide rates in Connecticut compared with its synthetic control and with all states in the control pool. Connecticut's nonfirearm homicide rate trend tracked closely with that of its synthetic control's prior to the PTP law's implementation. However, the nonfirearm homicide rates for

Connecticut and its synthetic control did not diverge following the law's implementation. Summing the differences between Connecticut and its synthetic control from 1996 to 2005, we estimated that the law was associated with 24 fewer nonfirearm homicides during this period than expected. The permutation tests indicated that any divergence between Connecticut's nonfirearm homicide rates and those of its synthetic control during the postlaw period was not statistically significant (Table 2).

DISCUSSION

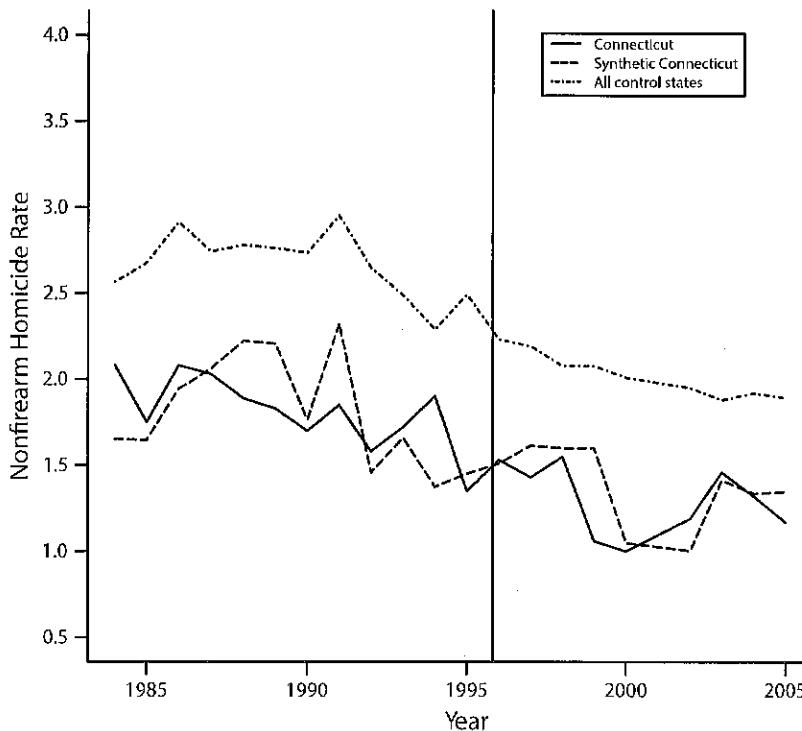
Previous studies have suggested that PTP laws may prevent the diversion of guns to criminals,¹³⁻¹⁵ and the sharp increase in gun homicides after Missouri's PTP law was repealed suggests that PTP laws may reduce lethal violence.¹⁶ Consistent with these previous studies, this study demonstrated that Connecticut's PTP law was associated with a subsequent reduction in homicide rates. As would be expected if the PTP law drove the reduction, the effects were only seen for homicides committed with firearms.

Connecticut's firearm homicide rate trend departed from its synthetic control from 1999 to 2005. This lag between the law's implementation and divergence in homicide trend may call into question whether the estimated effect resulted from the PTP law or from unmeasured interventions enacted in 1999 that only selectively reduced firearm homicides. However, there are plausible explanations for a delayed policy effect. First, spikes in gun sales may occur just prior to a significant gun control law, perhaps because of media scrutiny, and the additional guns sold under less rigorous regulation could temporarily counteract the law's preventive effects.^{17,18} Second, the number of transactions blocked by the PTP law may accumulate over time until gun availability in the underground market is sufficiently constrained to appreciatively affect handgun acquisition. The net effect of these 2 opposing forces—prelaw sales uptick and postlaw downturn—may result in no immediate effect but fewer high-risk gun acquisitions several years after implementation. Such a delayed effect was observed following Maryland's ban of small, poorly constructed handguns that were overrepresented in crime.¹⁸



Note. Connecticut (solid line) compared with synthetic Connecticut (dashed line) and all states in the control pool, equally weighted (dotted line). The vertical line indicates when Connecticut's permit-to-purchase law was implemented.

FIGURE 1—Firearm homicide rates: Connecticut, 1996–2005.



Note. Connecticut (solid line) compared with synthetic Connecticut (dashed line) and all states in the control pool, equally weighted (dotted line). The vertical line indicates when Connecticut's permit-to-purchase law was implemented. Rates for 2001 are not included because of the World Trade Center attacks.

FIGURE 2—Nonfirearm homicide rates: Connecticut, 1996–2005.

It is plausible that Connecticut's PTP law could reduce firearm homicide rates as substantially as the 40% reduction estimated. The PTP law (1) strengthened background check requirements for handguns sold by private sellers and licensed firearm dealers, (2) required completion of an approved handgun safety course of at least 8 hours, and (3) increased the minimum legal age for handgun purchase from 18 to 21 years, blocking an age group with a high homicide offending rate.¹⁹ Since 1965, Connecticut law has required private handgun sellers to mail a form to local police with information on prospective handgun purchasers to allow for—but not mandate—a background check with a 1-week waiting period. Local authorities with knowledge of a prospective purchaser's ineligibility to possess a handgun were required to notify the seller. This law was strengthened in October 1994 to require local law enforcement to "make a reasonable effort" to determine whether an applicant was ineligible to own a handgun

(Connecticut Public Act No. 94-1 [July Special Session 1994], Section 1[b]); in October 1995, it was further strengthened by the PTP law, which requires prospective handgun purchasers to obtain an eligibility certificate through their local police department. The implementation of the PTP law also changed the process for purchasing handguns from licensed firearm dealers—previously, handgun purchasers could apply for a permit directly from a gun shop. After the PTP law, if the applicant passed a background check and showed proof of successful completion of an approved handgun safety course, then a permit was issued that would be valid for 5 years. Requiring application in person at the police department as well as the safety course may dissuade potential straw purchasers (those who buy guns for prohibited persons) or others considering purchasing handguns to commit a crime.

The law's protective effects against homicides may be mediated by reductions in the

diversion of guns to criminals. These diversions are indirectly measured from traces of guns recovered by police such as crime guns that come across state borders and have short sale-to-crime intervals.²⁰ Unfortunately, reliable crime gun trace data do not extend to the prelaw period, so we could not test this hypothesis. Current crime gun trace indicators suggest that Connecticut is performing better than the national average in terms of gun diversions. The average sale-to-crime interval for guns recovered by police in Connecticut is more than 2.5 years longer than the national average.²¹ Almost half of the guns recovered by police in Connecticut originated from retail sales in other states, approximately 15% higher than the national average.²¹

Estimating state law effects requires estimating the counterfactual—the outcome had the law not been implemented but all else remained equal. This is typically done by comparing outcomes over time between states with the law and states without the law. The synthetic control method used in this study was appropriate for the comparative case study design and was related to the difference-in-differences approach to estimating intervention effects.⁹ This method has gained popularity recently in estimating economic and health policy effects.^{9,22–25} The advantages of this approach and its assumptions have been discussed previously.²⁶

The first assumption of the synthetic control approach is that there were no interruptions in the law and no effects prior to its implementation. There was no evidence that the law's implementation was interrupted. However, as

TABLE 2—Proportion of Control States With Results as Extreme as or More Extreme Than Connecticut: 1996–2005

| Control States Included ^a | Firearm | Nonfirearm |
|--------------------------------------|---------|------------|
| ≤ 20× MSPE | 3/38 | 13/39 |
| ≤ 5× MSPE | 0/30 | 11/32 |
| ≤ 2× MSPE | 0/24 | 8/26 |

Note. MSPE = mean squared prediction error.

^aResults from permutation tests including control states whose synthetic control's MSPE is ≤ 20×, 5×, and 2× that of the MSPE of Connecticut's synthetic control.

stated previously, it is plausible that more handguns were purchased just prior to the PTP law's implementation.

The second assumption is that the implementation of the PTP law has no effect on other states' homicide rates. If this assumption was violated in this study, there is no appealing strategy for relaxing it. One approach would be to restrict the analysis to states that are not geographically close to Connecticut. The drawback of this strategy is that states such as Rhode Island and New Hampshire, which were large contributors to Connecticut's synthetic control, would be excluded.

The third assumption is that there are no unmeasured confounders during the postlaw period. This is a concern in any study with nonrandom assignment to intervention status. However, the synthetic control provided a good fit to Connecticut's homicide rates during the prelaw period, and intrastate correlation of homicide rates from 1984 to 2005 was very high, ranging from 0.84 to 0.97. Thus, a synthetic control that fits well during the prelaw period is likely to provide a good fit during the postlaw period as well.

Connecticut passed 2 gun laws of note in the poststudy period. In 1998, Connecticut began prohibiting firearm possession for persons who committed serious offenses adjudicated in juvenile courts. However, this condition affected a very small segment of gun offenders who were not already prohibited, and there is no evidence that these policies affected homicide rates.²⁷ In 1999, Connecticut began requiring background checks for private transfers of long guns. However, long guns accounted for a small percentage of the firearms used in murders in Connecticut during the study period prior to 1999.²⁸

Rhode Island, which contributed most to the firearm homicide synthetic control (72%), did not adopt a significant gun law during the postlaw study period. Maryland, which accounted for 14% of the firearm homicide synthetic control, implemented a law in October 1996 that required background checks for all handgun transfers. This law, in addition to a 1990 ban of "junk guns," may have reduced firearm homicides in Maryland.¹⁸ California contributed less than 5% of the firearm homicide synthetic control and was active in adopting stricter gun control laws throughout the study period, the most significant of which

were comprehensive background checks for handgun transfers and 10-year firearm prohibitions for violent misdemeanants. Both were implemented in 1991. Any protective effects of firearm laws in Maryland and California that were realized after 1995 may have biased our estimates of the impact of Connecticut's PTP law on firearm homicide rates toward the null. Successful interventions in major jurisdictions in the states included in the synthetic control could have confounded our estimates. However, we are unaware of any intervention that affected firearm homicides enough to have affected statewide rates over a 7-year period.

Fixed effects regression models are a common way of estimating the effects of state laws while also controlling for variables that may have potentially confounded this estimate. We believed this approach to be inappropriate in this case for several reasons. First, it relied on questionable assumptions that all states and time periods could have implemented a PTP law and that the association between PTP law implementation and homicide rates would be the same for all states. We had very little data with which to evaluate these assumptions, because only one other state implemented a PTP law during the study period. (Nebraska implemented a PTP law in 1991 that differed in important ways from Connecticut's.) In addition, fixed effects regression models failed to recognize the comparative case study design of both the data and research question and would have inappropriately extrapolated the effect estimated for Connecticut to the pool of control states.

The goal of this study was to estimate the effect of Connecticut's PTP law on homicides in Connecticut—not to extrapolate the effect of Connecticut's law on homicides to an average control state. The synthetic control approach allowed us to estimate such an effect and appropriately restricted the interpretation to the state of Connecticut. In addition, the method of assessing significance of the estimated results was more appropriate than a large-sample inferential technique, such as regression, given the small number of units.⁹ Other advantages of this method over standard regression methods included (1) the data-driven estimation of policy effects (through the synthetic control weights) to produce the most accurate counterfactual and (2) the

incorporation of both graphical and numerical checks (through the MSPE) of how well the comparison approximated the case.

Examining the extent to which stronger background check policies affect suicide rates is an area for future work. Previous research suggests that states with stricter gun permitting and licensing regulations have lower suicide rates.²⁹ This research should be corroborated with studies that use longitudinal data to examine changes in PTP laws and subsequent changes in firearm suicide rates.

This study has important policy implications as lawmakers consider options for reducing gun violence. Connecticut's PTP law seems to reduce firearm-specific homicides. Following the process in place in 6 states now, the most recent federal legislation considered by Congress to require background checks for many private party transactions would require prospective purchasers to go to a federally licensed gun dealer who would process the purchase application and submit the information for the background check. Future research should compare the effectiveness of this approach versus the approach used in PTP laws. Other unexamined issues include standards of evidence to hold noncompliant gun sellers accountable and the significance of penalties for failing to comply with gun sales laws. ■

About the Authors

Kara E. Rudolph is with the School of Public Health, University of California, Berkeley; Center for Health and Community, University of California, San Francisco; and Department of Mental Health, Bloomberg School of Public Health, Johns Hopkins University, Baltimore, MD. **Elizabeth A. Stuart** is with the Departments of Mental Health and Biostatistics, Bloomberg School of Public Health, Jon S. Vernick and Daniel W. Webster are with the Center for Gun Policy and Research, Bloomberg School of Public Health.

Correspondence should be sent to Daniel W. Webster, 624 N. Broadway, Room 593, Baltimore, MD 21205 (e-mail: dwwebste2@jhu.edu). Reprints can be ordered at <http://www.ajph.aphapublications.org> by clicking the "Reprints" link.

This article was accepted April 4, 2015.

Contributors

K. E. Rudolph contributed to the study design and interpretation of results and led the analysis, drafting, and revision of the article. E. A. Stuart contributed to the study design, analysis, interpretation of results, and article revisions. J. S. Vernick contributed to obtaining the data, interpreting the results, and revising the article. D. W. Webster conceptualized the study and contributed to obtaining the data, interpreting the results, and drafting and revising the article.

RESEARCH AND PRACTICE

Acknowledgments

Funding for this project came from a grant from the Joyce Foundation. K. E. Rudolph's time was also funded by Drug Dependence Epidemiology Training grant T32DA007292-21 (Debra Furr-Holden, principal investigator) and by the Robert Wood Johnson Foundation Health & Society Scholars program.

Note. The views and opinions expressed in this article are those of the authors and should not be construed as representing the views of any of the sponsoring organizations, agencies, or US government. The authors claim no conflicts of interest.

Human Participant Protection

This study was determined not to be human participant research by the institutional review board at the Johns Hopkins Bloomberg School of Public Health.

References

1. Centers for Disease Control and Prevention, National Center for Injury Prevention and Control. Leading cause of death reports, national and regional, 1999–2011. Available at: http://webappa.cdc.gov/sasweb/ncipc/leadcaus10_us.html. Accessed September 12, 2014.
2. Kochanek KD, Arias E, Anderson RN. How did cause of death contribute to racial differences in life expectancy in the United States in 2010? *NCHS Data Brief*. No. 125. Hyattsville, MD: National Center for Health Statistics; 2013.
3. The Federal Bureau of Investigation. Crime in the United States 2012. Available at: http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u-s/2012/crime-in-the-u-s-2012/offenses-known-to-law-enforcement/expanded-homicide/expanded_homicide_data_table_7_murder_types_of_weapons_used_percent_distribution_by_region_2012.xls. Accessed October 23, 2014.
4. Wintemute GJ, Drake CM, Beaumont JJ, Wright MA, Parham CA. Prior misdemeanor convictions as a risk factor for later violent and firearm-related criminal activity among authorized purchasers of handguns. *JAMA*. 1998;280(24):2083–2087.
5. Campbell JC, Webster D, Koziol-McLain J, et al. Risk factors for femicide in abusive relationships: results from a multisite case control study. *Am J Public Health*. 2003; 93(7):1089–1097.
6. Anglemeyer A, Horvath T, Rutherford G. The accessibility of firearms and risk for suicide and homicide victimization among household members. *Ann Intern Med*. 2014;160(2):101–110.
7. Miller M, Hemenway D, Azrael D. State-level homicide victimization rates in the US in relation to survey measures of household firearm ownership, 2001–2003. *Soc Sci Med*. 2007;64(3):656–664.
8. Law Center to Prevent Gun Violence. Universal background checks and the private sale loophole policy summary. Available at: <http://smartgunlaws.org/universal-gun-background-checks-policy-summary>. Accessed September 12, 2014.
9. Abadie A, Diamond A, Hainmueller J. Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program. *J Am Stat Assoc*. 2010;105(490):493–505.
10. Abadie A, Diamond A, Hainmueller J. Synth: An R package for synthetic control methods in comparative case studies. *J Stat Softw*. 2011;42(13):1–17.
11. Rosenbaum P. *Design of Observational Studies*. New York, NY: Springer; 2010.
12. R Core Team. *R: A Language and Environment for Statistical Computing*. Vienna, Austria: R Foundation for Statistical Computing; 2013.
13. Webster DW, Vernick JS, Bulzacchelli MT. Effects of state-level firearm seller accountability policies on firearm trafficking. *J Urban Health*. 2009;86(4):525–537.
14. Webster DW, Vernick JS, McGinty EE, Alcorn T. Preventing the diversion of guns to criminals through effective firearm sales laws. In: Webster DW and Vernick JS, eds. *Reducing Gun Violence in America: Informing Policy With Evidence and Analysis*. Baltimore, MD: Johns Hopkins University Press; 2013:109–122.
15. Flegler EW, Lee LK, Monuteaux MC, Hemenway D, Mannix R. Firearm legislation and firearm-related fatalities in the United States. *JAMA Intern Med*. 2013; 173(9):732–740.
16. Webster D, Crifasi CK, Vernick JS. Effects of the repeal of Missouri's handgun purchaser licensing law on homicides. *J Urban Health*. 2014;91(2):293–302.
17. Koper CS. America's experience with the federal assault weapons ban, 1994–2004: key findings and implications. In: Webster DW and Vernick JS, eds. *Reducing Gun Violence in America: Informing Policy With Evidence and Analysis*. Baltimore, MD: Johns Hopkins University Press; 2013:157–171.
18. Webster DW, Vernick JS, Hepburn LM. Effects of Maryland's law banning "Saturday night special" handguns on homicides. *Am J Epidemiol*. 2002;155(5): 406–412.
19. Braga AA, Wintemute GJ, Pierce GL, Cook PJ, Ridgeway G. Interpreting the empirical evidence on illegal gun market dynamics. *J Urban Health*. 2012;89(5): 779–793.
20. The Bureau of Alcohol, Tobacco, Firearms, and Explosives. Firearms trace data. 2013. Available at: <http://www.atf.gov/content/About/statistics/firearms-trace-data-2013>. Accessed March 3, 2015.
21. The Federal Bureau of Investigation. Crime in the United States 2011. Available at: <http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u-s/2011/crime-in-the-u-s-2011/tables/expanded-homicide-data-table-3>. Accessed September 12, 2014.
22. Billmeier A, Nannicini T. Assessing economic liberalization episodes: a synthetic control approach. *Rev Econ Stat*. 2013;95(3):983–1001.
23. Coffman M, Noy I. Hurricane Iniki: measuring the long-term economic impact of a natural disaster using synthetic control. *Environ Dev Econ*. 2012;17(2): 187–205.
24. Abadie A, Gardeazabal J. The economic costs of conflict: a case study of the Basque Country. *Am Econ Rev*. 2003;93(1):113–132.
25. Bauhoff S. The effect of school district nutrition policies on dietary intake and overweight: a synthetic control approach. *Econ Hum Biol*. 2014;12(Jan):45–55.
26. Abadie A, Diamond A, Hainmueller J. Comparative politics and the synthetic control method. *Am J Pol Sci*. 2015;59(2):495–510.
27. Vittes KA, Vernick JS, Webster DW. Legal status and source of offenders' firearms in states with the least stringent criteria for gun ownership. *Inj Prev*. 2013;19(1): 26–31.
28. Connecticut Department of Public Safety, Division of State Police. Uniform crime reports. Crime in Connecticut 1998 annual report. Available at: <http://www.dpsdata.ct.gov/dps/ucr/data/1998/Crime%20in%20Connecticut%201998.pdf>. Accessed March 3, 2015.
29. Rodriguez Andrés A, Hempstead K. Gun control and suicide: the impact of state firearm regulations in the United States, 1995–2004. *Health Policy*. 2011;101(1):95–103.

Copyright of American Journal of Public Health is the property of American Public Health Association and its content may not be copied or emailed to multiple sites or posted to a listserv without the copyright holder's express written permission. However, users may print, download, or email articles for individual use.